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METHODS OF DEALING WITH THE QUESTION OF TEMPERATURE IN THE COMPARISON OF STANDARDS OF LENGTH.

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It has been the experience of the writer that when two units of length traced upon the same bar of metal are compared, the probable error of a single comparison is about 0.2μ or, approximately, $\frac{1}{125000}$ inch. If the same units are traced upon bars of metal having widely different coefficients of expansion, the probable error of a single comparison is at least five times this amount. The conclusion to be drawn from this fact is, either that the thermometer used to measure the temperature of the metal fails to give the real temperature of the air which surrounds the bar, or that it fails to give the mean temperature of the entire mass of the metal. The mercurial thermometer simply registers the temperature of the mercury enclosed in the bulb. When placed upon the surface of a bar of metal it registers the temperature of the surface of the bar only when there is an equalization between the surface temperature of the metal and the entire mass of the mercury by mutual conduction.

Whenever slow changes in temperature take place, the thermometer may be relied upon to give tolerably accurate indications of the real temperature both of the air and of any metal surface with which it may be brought in contact. But when there is a sudden change of temperature, even if slight in amount, it is at least an open question whether the indications given by the thermometer are real for two or three minutes after the change takes place. The effect for the first instant will be to give an indication the reverse of that which would naturally be expected, on account of the shrinkage or the expansion of the glass bulb. Again, the entire mass of the mercury within the bulb does not take a uniform temperature instantly. It is probable that the outer layer of mercury in the bulb is heated or cooled considerably in advance of the point of a homogeneous temperature for the entire mass. The effect of this action will always be to give too great readings during the first moments of a sudden rise of temperature and too small whenever a sudden fall takes place. All such indications are therefore abnormal and unreliable. Accordingly, it is always my practice to read the thermometers at the instant of entering the comparing-room, and assume that this reading indicates the temperature at which the subsequent comparisons of any unit of length are made; always taking care that the time occupied shall be so short that the effect of any rise of temperature due to the presence of the observer cannot be sensible in any change in the length of the standards compared.

If an even temperature can be maintained for a certain length of time, depending on the masses of the bars of metal compared, it is probable that the thermometer resting upon the upper surface of the bars will indicate the real temperature of the mean mass of the bars. The length of time required will depend not only upon the mass, but upon the shape of the bars. In the case of a brass bar forty inches in length and one inch in cross-section, the time demanded in the case of a change of a few degrees only is from four to five hours. For the same metal one inch wide and $\frac{1}{100}$ inch thick this time is reduced to one or two minutes. It cannot be too strongly stated that the basis of certainty in the comparisons of standards of length is the knowledge that the temperature of the entire mass of metal is the same as that indicated by the thermometer when the comparisons are made.

It is just here that careful work is needed in order to reduce metrology to an exact science. In general terms, the problem may be stated as follows: Given a mass of metal of known shape and dimensions, upon which line graduations are traced, how much time must elapse after a known change of temperature takes place before the effect of this change shall produce its normal effect in changing the length of the bar? An approximate determination of this interval of time has been made for my standards upon glass, upon steel and upon bronze.

It will be readily seen that the method of procedure above described must be uncertain and unsatisfactory. First of all, it was found impossible in all of my earlier work to maintain even an approximately constant temperature for even a short time. After the completion of the comparing-room at Harvard Hall, there was a most decided improvement in this respect. But it was only after the construction of the new room beneath the rotunda of the Observatory, from the avails of an appropriation from the Rumford Committee of the American Academy of Arts and Sciences, that real progress began in the study of this problem. This room is built inside of another room, which is surrounded by an enormous mass of stone. There is a clear space on every side of the room. The walls, the floor and the ceiling are double. To fill the intervening spaces required seventeen barrels of sawdust. The room is heated by an admirable piece of apparatus furnished by the Perfect Hatcher Co. of Elmira, N. Y. The temperature is regulated by a thermostat furnished by the same company.

In the new comparing-room, it has been found possible to produce low and steady increments and decrements of temperature, and I have also been able to maintain a temperature never varying from the mean value more than two- or three-tenths of a degree for several days together.

Since this paper is intended only as a preliminary notice of the experiments undertaken by Mr. A. McRae, of the United States Signal Service and myself, it will only be proper to simply refer in a general way to the results thus far obtained.

First—The observations of previous years with respect to the impunity with which one can neglect the effect of any change of temperature due to the presence of the observer in producing a change of length in a standard having a comparatively large mass have been fully confirmed. In the case of my bronze standard, 41×1×1 inches, a rise of 1° C. is never perceptible until after the lapse of from twelve to fifteen minutes. On the other hand, with an aluminum strip one inch wide and $\frac{1}{100}$ inch thick, the change takes place from one to five minutes before it is indicated by the thermometer.

Second—The conclusion stated above rests upon the assumption that the changes are produced by air contact only. When conduction by contact with a foreign substance takes place, entirely different phenomena are observed. The action which now takes place will be best understood by describing briefly one series of experiments.

An end-measure meter was placed between the fixed stop and the movable stop attached to the microscope carriage of the comparator. The microscope attached to the carriage was then set for coincidence with any line upon a second bar placed upon the opposite side of the comparator. It is obvious that any increase in the length of the end-measure standard would be seen instantly by the motion of the line under the microscope, as the bar would push the carriage before it. Now, if I place the palms of my hands upon the upper surface of the bar for, *e.g.*, one minute, I observe an *instantaneous* increase in length. Call this increase m. If, after the lapse of five minutes,I again place my hands upon the bar for one minute the increase in length will be nearly 2m. At the end of ten contacts the increase will be 10m nearly, and this final length will remain nearly constant for about half an hour. The whole amount of heat imparted will be dissipated in about four hours.

Thus far, I find myself unable to resist the conclusion that the action described above is purely a surface action. It hardly seems possible that the upper surface of a bar can expand while the central part remains at the same length; but one may be forced to this conclusion, especially since it can be proved that no warping of the surface takes place in this case.

Third—There is some evidence that the amount of expansion and of contraction is in some way connected with the amount of moisture in the air with which the standards are surrounded.

Fourth—Even when the temperature seems to be constant, it really has a *drift* either up or down. In all the comparisons which have been made since 1883, the aim has been to combine the observations in such a manner that the error due to *drift* shall be eliminated.

Fifth—When the errors due to local drift have been taken into account, there remains pretty clear evidence that there are residual errors which are a function of the season of the year at which the observations are made. In other words, the errors of short period due to local drift are supplemented by errors of long period which naturally follow the season of the year. At present this view must be taken simply as a working hypothesis. My attention was first called to this matter by the deviation between the summer and the winter comparisons of a standard made for Professor Brackett of Princeton College. In the early part of 1884, three standard yards and meters upon Jessup's steel were constructed simultaneously. One of these went to Professor Brackett, one to Professor Rowland, and the other was retained for my own use. Since there were indications of a change of length in my own bar as well as in the one sent to Professor Brackett, Professor Rowland, at my request, kindly returned his bar for a re-examination several months after it was originally sent to him.

The various relations found for these two bars are given below, A_0 representing the *Metre des Archives*, and Y_0 the Imperial Yard:

ROGERS' YARD AND METER.

Date of Comparisons.	Equations.
1884, Feb. 5—Mar. 11,	$R_3+4.0\mu=A_0$
" " — "	$R_3+4.1\mu=A_0$
" " — "	$R_3+2.4\mu=A_0$
" " — "	$R_3+2.7\mu=A_0$
" Nov. 16—Dec. 21,	$R_3-1.2\mu=A_0$
" Dec. 21—Dec. 31,	$R_3+2.2\mu=A_3$
1885, Jan. 20—Feb. 15,	$R_3+1.9\mu=A_0$
" May 25—July 8,	$R_3+2.3\mu=A_0$
1884, Feb. 5—Mar. 11,	$R_3+1.0\mu=Y$
" Mar. 12—Mar. 27,	$R_3-1.4\mu=Y$
" Feb. 5—Mar. 11,	$R_3-0.6\mu=Y$
" Mar. 9—Mar. 27,	$R_3-0.8\mu=Y$
1885, Jan. 12—Feb. 16,	$R_3-0.5\mu=Y$

ROWLAND'S YARD AND METER.

Date of Comparisons.	Equations.
1884, Feb. 5—Mar. 11,	$A_4+1.2\mu=A_0$
" Mar. 12—Mar. 21,	$A_4+1.3\mu=A_0$
1885, Jan. 20—Feb. 15,	$A_4-0.9\mu=A_0$
1884, Feb. 5—Mar. 5,	$A_4-0.4\mu=A_0$
" " — "	$A_4-0.0\mu=A_0$
1884, Feb. 5—Mar. 11,	$A_4+1.8\mu=Y$
" Mar. 12—Mar. 27,	$A_4+0.3\mu=Y$
1885, Jan. 12—Feb. 24,	$A_4+1.8\mu=Y$
1884, Feb. 5—Mar. 5,	$A_4+1.4\mu=Y$
" Mar. 9—Mar. 27,	$A_4+0.2\mu=Y$

With the exception of the observations of Meter R_3, for Nov. 16 to Dec. 4, the disagreement of the various determinations can probably be accounted for by errors of observation. In this case, however, the usual care in securing an elimination of errors due to drift was *not* taken. The same remark applies to the first comparison of Professor Brackett's standard, where the deviation from the relation found subsequently is five mikrons. As soon as the errors of long period began to be suspected, a careful series of comparisons of my bronze and steel standards was begun in the new comparing-room, under conditions very favorable to their detection. Under a continuously varying temperature, the errors of long period will always be combined with the errors of short period. The varying relations of the steel bar, R_3 to A_0, will be seen from the following table:

No. Obs.	1885.		
10	May 25 to May 27,	10.1° C.	$R_3+3.6\mu=A_0$
10	May 27 to May 31,	11.6	$R_3+2.6\mu=A_0$
10	May 31 to June 2,	10.7	$R_3+3.2\mu=A_0$
10	June 3 to June 4,	11.2	$R_3+2.4\mu=A_0$
10	June 5 to June 16,	12.6	$R_3+2.4\mu=A_0$
10	June 21 to June 27,	14.1	$R_3+0.6\mu=A_0$
10	June 25 to July 8,	15.1	$R_3+0.7\mu=A_0$

If the probable certainty of these errors of long period can be established, it will go far toward clearing up the doubt which at present exists concerning the change of length in standards due to their age. The relation between the Washington Yard Bronze 11 and the Imperial Yard at London has varied, according to different observers, between about eight hundred-thousandths of an inch too long in 1855, eighty-eight millionths of an inch too short in 1878, and twenty-two millionths of an inch too short in 1883. These deviations are quite within the range of the errors of long period, which, according to my present experience, one ought to expect.

It is the object of the experiments undertaken by Mr. McRae and myself to find some way by which the observers can be sure that the indicated temperature represents the real temperature of the entire mass of the bar of metal upon which a standard of length is traced. To accomplish this, four distinctly different classes of simultaneous observations were made.

First—The thermometer, placed upon the upper surface of the bar, was read in the usual way.

Second—A strip of steel and a strip of aluminum were fastened at one end of the comparator, side by side, passing over rollers at the other end and fastened to the wall of the comparing-room at a point a little below the plane of the strips. Two spring scales intervene to give the required tension to each strip. These strips are forty inches long, one inch wide, and .016 inch thick. At the distance of one meter from the end at which the fixed junction is made, a line was traced across the face of each strip, the temperature at that time being about 13° C. By the aid of the micrometer of a microscope mounted upon the movable carriage of the comparator, the amount of the separation of these lines either way can be measured. The micrometer readings of these lines are carried on simultaneously with the thermometer readings.

Third—It is well known that if copper and German silver wires form one terminal of a mechanical junction, and that if a galvanometer is placed in the circuit at another junction of the same metals, kept at a constant temperature, *viz.*, by submergence in melting ice, the difference in the temperature of the two junctions will be instantly indicated upon the galvanometer scale. In the present experiment, 1° C. was found to be 16 mms.; hence it was possible to read directly to $\frac{1}{16}$°, and by estimation, easily to $\frac{1}{160}$°.

In a general way this method does not seem to possess the degree of reliability of which it at first appeared to be capable, but for measuring small differences of temperature nothing better could be desired. By making one junction insulated at the center of the bar, whose mean temperature is desired, and making the other junction first at the surface and then at the different points in the air surrounding the bar, it is easy to determine when there is an equilibrium of temperature between the medium through which the heat is supplied; *viz.*, the surrounding air and the interior of the mass of the bar whose mean temperature is desired. The decided advantage which this method of measuring temperature possesses is that the indications are instantaneous in their action.

Fourth—From a somewhat extended examination of the performance of the form of metal thermometers made by the Standard Thermometer Co., of Peabody, Mass., I became convinced that the reliability of the indicated temperature was much greater than I had previously supposed. The evidence is fairly conclusive that for indicating *the temperature of the surrounding air* they are comparable with the best standard mercurial thermometers. In order to test the extent of their reliability, the Company made from my designs a form of microscopic attachment by which it is easy to ascertain what the indicated temperatures really are, probably within $\frac{1}{100}$, and perhaps within $\frac{1}{200}$ of a degree.

We have now four radically different methods of indicating the temperature of the bar whose mean temperature is desired. When all of these methods agree in giving the same result, we may be reasonably sure that the conditions are favorable for the comparison of the standards of length. The comparisons are all made as quickly as possible, generally within three or four minutes, and then the observer quits the room and does not return for at least three or four hours; not until the heat developed by the presence of his person has been dissipated and merged in the temperature of the entire mass of the metals. The value of the comparisons will depend as much upon the ability of the observer to detect the proper time at which to make the comparisons, as upon the skill with which they are made. In fact, after an observer has become accustomed to this class of observations, his success will depend largely upon his being able to decide when the observations should *not* be made.